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British Association for the Advancement of Science.

MONTREAL, 1884.

ADDRESS

BY

THE RIGHT HON. LORD RAYLEIGH,

M.A., D.C.L., F.R.S., F.R.A.S., F.R.G.S., *Professor of Experimental Physics in the University of Cambridge*,

PRESIDENT.

LADIES AND GENTLEMEN—

IT is no ordinary meeting of the British Association which I have now the honour of addressing. For more than fifty years the Association has held its autumn gathering in various towns of the United Kingdom, and within those limits there is, I suppose, no place of importance which we have not visited. And now, not satisfied with past successes, we are seeking new worlds to conquer. When it was first proposed to visit Canada, there were some who viewed the project with hesitation. For my own part, I never quite understood the grounds of their apprehension. Perhaps they feared the thin edge of the wedge. When once the principle was admitted, there was no knowing to what it might lead. So rapid is the development of the British Empire, that the time might come when a visit to such out-of-the-way places as London or Manchester could no longer be claimed as a right, but only asked for as a concession to the susceptibilities of the English. But seriously, whatever objections may have at first been felt soon were outweighed by the consideration of the magnificent opportunities which your hospitality affords of extending the sphere of our influence and of becoming acquainted with a part of the Queen's dominion which, associated with splendid memories of the past, is advancing daily by leaps and bounds to a position of importance such as not long ago was scarcely dreamed of. For myself, I am not a stranger to your shores. I remember well the impression made upon me, seventeen years ago, by the wild rapids of the St. Lawrence, and the gloomy grandeur of the Saguenay. If anything impressed me more, it was the kindness with which I was received by yourselves, and which I doubt not will be again extended not merely to myself but to all the English members of the Association. I am confident that those who have made up their minds to cross the ocean will not repent their decision, and that, apart altogether from scientific interests, great advantage may be expected from this visit. We Englishmen ought to know more than we do of matters relating to the Colonies, and anything which tends to bring the various parts of the Empire into closer contact can hardly be overvalued. It is pleasant to think that this Association is the means of furthering an object which should be dear to the hearts of all of us; and I venture to say that a large proportion of the visitors to this country will be astonished by what they see, and will carry home an impression which time will not readily efface.

To be connected with this meeting is, to me, a great honour, but also a great responsibility. In one respect, especially, I feel that the Association might have done well to choose another President. My own tastes have led me to study mathematics and physics rather than geology and biology, to which naturally more attention turns in a new country, presenting as it does a fresh field for investigation. A chronicle of achievements in these departments by workers from among yourselves would have been suitable to the occasion, but could not come from me. If you would have preferred a different subject for this address, I hope, at least, that you will not hold me entirely responsible.

At annual gatherings like ours the pleasure with which friends meet friends again is sadly marred by the absence of those who can never more take their part in our proceedings. Last year my predecessor in this office had to lament the untimely loss of Spottiswoode and Henry Smith, dear friends of many of us, and prominent members of our Association. And now, again, a well-known form is missing. For many years Sir W. Siemens has been a regular attendant at our meetings, and to few indeed have they been more indebted for success. Whatever the occasion, in his Presidential Address of two years ago, or in communications to the Physical and Mechanical Sections, he had always new and interesting ideas, put forward in language which a child could understand, so great a master was he of the art of lucid statement in his adopted tongue. Practice with Science was his motto. Deeply engaged in industry, and conversant, all his life, with engineering operations, his opinion was never that of a mere theorist. On the other hand, he abhorred rule of thumb, striving always to master the scientific principles which underlie rational design and invention.

It is not necessary that I should review in detail the work of Siemens. The part which he took, during recent years, in the development of the dynamo machine must be known to many of you. We owe to him the practical adoption of the method, first suggested by Wheatstone, of throwing into a shunt the coils of the field magnets, by which a greatly improved steadiness of action is obtained. The same characteristics are observable throughout—a definite object in view and a well-directed perseverance in overcoming the difficulties by which the path is usually obstructed.

These are, indeed, the conditions of successful invention. The world knows little of such things, and regards the new machine or the new method as the immediate outcome of a happy idea. Probably, if the truth were known, we should see that, in nine cases out of ten, success depends as much upon good judgment and perseverance as upon fertility of imagination. The labours of our great inventors are not unappreciated, but I doubt whether we adequately realise the enormous obligations under which we lie. It is no exaggeration to say that the life of such a man as Siemens is spent in the public service; the advantages which he reaps for himself being as nothing in comparison with those which he confers upon the community at large.

As an example of this it will be sufficient to mention one of the most valuable achievements of his active life—his introduction, in conjunction with his brother, of the Regenerative Gas Furnace, by which an immense economy of fuel (estimated at millions of tons annually) has been effected in the manufacture of steel and glass. The nature of this economy is easily explained. Whatever may be the work to be done by the burning of fuel, a certain *temperature* is necessary. For example, no amount of heat in the form of boiling water, would be of any avail for the fusion of steel. When the products of combustion are cooled down to the point in question, the heat which they still contain is useless as regards the purpose in view. The importance of this consideration depends entirely upon the working temperature. If the object be the evaporation of water or the warming of a house, almost all the heat may be extracted from the fuel without special arrangements. But it is otherwise when the temperature required is not much below that of combustion itself, for then the escaping gases carry away with them the larger part of the whole heat developed. It was to meet this difficulty that the regenerative furnace was devised. The products of combustion, before dismissal into the chimney, are caused to pass through piles of loosely stacked fire-brick, to which they give up their heat. After a time the fire-brick, upon which the gases first impinge, becomes nearly as hot as the furnace itself. By suitable valves the burnt gases are then diverted through another stack of brickwork which they heat up in like manner, while the heat stored up in the first stack is utilised to warm the unburnt gas and air on their way to the furnace. In this way almost all the heat developed at a high temperature during the combustion is made available for the work in hand.

As it is now several years since your presidential chair has been occupied by a professed physicist, it may naturally be expected that I should attempt some record of recent progress in that branch of science, if indeed such a term be applicable. For it is one of the difficulties of the task that subjects as distinct as Mechanics, Electricity, Heat, Optics and Acoustics to say nothing of Astronomy and Meteorology, are included under Physics Any one of these may well occupy the life-long attention of a man of science, and to be thoroughly conversant with all of them is more than can be expected of any one individual, and is probably incompatible with the devotion of much time and energy to the actual advancement of knowledge. Not that I would complain of the association sanctioned by common parlance. A sound knowledge of at least the principles of general physics is necessary to the cultivation of any department. The predominance of the sense of sight as the medium of communication with the outer world, brings with it dependence upon the science of optics; and there is hardly a branch of science in which the effects of *temperature* have not (often without much success) to be reckoned with. Besides, the neglected borderland between two branches of knowledge is often that which best repays cultivation, or, to use a metaphor of Maxwell's, the greatest benefits may be derived from a cross fertilisation of the sciences. The wealth of material is an evil only from the point of view of one of whom too much may be expected. Another difficulty incident to the task, which must be faced but cannot be overcome, is that of estimating rightly the value, and even the correctness, of recent work. It is not always that which seems at first the most important that proves in the end to be so. The history of science teems with examples of discoveries which attracted little notice at the time, but afterwards have taken root downwards and borne much fruit upwards.

One of the most striking advances of recent years is in the production and application of electricity upon a large scale—a subject to which I have already had occasion to allude in connection with the work of Sir W. Siemens. The dynamo machine is indeed founded upon discoveries of Faraday now more than half a century old; but it has required the protracted labours of many inventors to bring it to its present high degree of efficiency. Looking back at the matter, it seems strange that progress should have been so slow. I do not refer to details of design, the elaboration of which must always, I suppose, require the experience of actual work to indicate what parts are structurally weaker than they should be, or are exposed to undue wear and tear. But with regard to the main features of the problem, it would almost seem as if the difficulty lay in want of faith. Long ago it was recognised that electricity derived from chemical action is (on a large scale) too expensive a source of mechanical power, notwithstanding the fact that (as proved by Joule in 1846) the conversion of electrical into mechanical work can be effected with great economy. From this it is an evident consequence that electricity may advantageously be obtained from mechanical power; and one cannot help thinking that if the fact had been borne steadily in mind, the development of the dynamo might have been much more rapid. But discoveries and inventions are apt to appear obvious when regarded from the standpoint of accomplished fact; and I draw attention to the matter only to point the moral that we do well to push the attack persistently when we can be sure beforehand that the obstacles to be overcome are only difficulties of contrivance, and that we are not vainly fighting unawares against a law of Nature.

The present development of electricity on a large scale depends, however, almost as much upon the incandescent lamp as upon the dynamo. The success of these lamps demands a very perfect vacuum—not more than about one-millionth of the normal quantity of air should remain,—and it is interesting to recall that, twenty years ago, such vacua were rare even in the laboratory of the physicist. It is pretty safe to say that these wonderful results would never have been accomplished had practical applications alone been in view. The way was prepared by an army of scientific men whose main object was the advancement of knowledge, and who could scarcely have imagined that the processes which they elaborated would soon be in use on a commercial scale and entrusted to the hands of ordinary workmen.

When I speak in hopeful language of practical electricity, I do not forget the disappointment within the last year or two of many over-sanguine expectations. The enthusiasm of the inventor and promoter are necessary to progress, and it seems to be almost a law of nature that it should overpass the bounds marked out by reason and experience. What is most to be regretted is the advantage taken by speculators of the often uninstructed interest felt by the public in novel schemes by which its imagination is fired. But looking forward to the future of electric lighting, we have good ground for encouragement. Already the lighting of large passenger ships is an assured success, and one which will be highly appreciated by those travellers who have experienced the tedium of long winter evenings unrelieved by adequate illumination. Here, no doubt, the conditions are in many respects especially favourable. As regards space, life on board ship is highly concentrated; while unity of management and the presence on the spot of skilled engineers obviate some of the difficulties that are met with under other circumstances. At present we have no experience of a house-to-house system of illumination on a great scale and in competition with cheap gas; but preparations are already far advanced for trial on an adequate scale in London. In large institutions, such as theatres and factories, we all know that electricity is in successful and daily extending operation.

When the necessary power can be obtained from the fall of water, instead of from the combustion of coal, the conditions of the problem are far more favourable. Possibly the severity of your winters may prove an obstacle, but it is impossible to regard your splendid river without the thought arising that the day may come when the vast powers now running to waste shall be bent into your service. Such a project demands of course the most careful consideration, but it is one worthy of an intelligent and enterprising community.

The requirements of practice react in the most healthy manner upon scientific electricity. Just as in former days the science received a stimulus from the application to telegraphy, under which everything relating to measurement on a small scale acquired an importance and development for which we might otherwise have had long to wait, so now the requirements of electric lighting are giving rise to a new development of the art of measurement upon a large scale, which cannot fail to prove of scientific as well as practical importance. Mere change of scale may not at first appear a very important matter, but it is surprising how much modification it entails in the instruments, and in the processes of measurement. For instance, the resistance coils on which the electrician relies in dealing with currents whose maximum is a fraction of an ampere, fail altogether when it becomes a question of hundreds, not to say thousands, of amperes.

The powerful currents, which are now at command, constitute almost a new weapon in the hands of the physicist. Effects, which in old days were rare and difficult of observation, may now be produced at will on the most conspicuous scale. Consider for a moment Faraday's great discovery of the 'Magnetisation of Light,' which Tyndall likens to the Weisshorn among mountains, as high, beautiful, and alone. This judgment (in which I fully concur) relates to the scientific aspect of the discovery, for to the eye of sense nothing could have been more insignificant. It is even possible that it might have eluded altogether the penetration of Faraday, had he not been provided with a special quality of very heavy glass. At the present day these effects may be produced upon a scale that would have delighted their discoverer, a rotation of the plane of polarization through 180° being perfectly feasible. With the aid of modern appliances, Kundt and Röntgen in Germany, and H. Becquerel in France, have detected the rotation in gases and vapours, where, on account of its extreme smallness, it had previously escaped notice.

Again, the question of the magnetic saturation of iron has now an importance entirely beyond what it possessed at the time of Joule's early observations. Then it required special arrangements purposely contrived to bring it into prominence. Now in every dynamo machine, the iron of the field-magnets approaches a state of saturation, and the very elements of an explanation of the action require us to take the fact into account. It is indeed probable that a better knowledge of this subject might lead to improvements in the design of these machines.

Notwithstanding the important work of Rowland and Stoletow, the whole theory of the behaviour of soft iron under varying magnetic conditions is still somewhat obscure. Much may be hoped from the induction balance of Hughes, by which the marvellous powers of the telephone are applied to the discrimination of the properties of metals, as regards magnetism and electric conductivity.

The introduction of powerful alternate-current in machines by Siemens, Gordon, Ferranti, and others, is likely also to have a salutary effect in educating those so-called practical electricians whose ideas do not easily rise above ohms and volts. It has long been known that when the changes are sufficiently rapid, the phenomena are governed much more by induction, or electric inertia, than by mere resistance. On this principle much may be explained that would otherwise seem paradoxical. To take a comparatively simple case, conceive an electro-magnet wound with two contiguous wires, upon which acts a given rapidly periodic electro-motive force. If one wire only be used, a certain amount of heat is developed in the circuit. Suppose now that the second wire is brought into operation in parallel—a proceeding equivalent to doubling the section of the original wire. An electrician accustomed only to constant currents would be sure to think that the heating effect would be doubled by the change, as much heat being developed in each wire separately as was at first in the single wire. But such a conclusion would be entirely erroneous. The total current, being governed practically by the self-induction of the circuit, would not be augmented by the accession of the second wire, and the total heating effect, so far from being doubled, would, in virtue of the superior conductivity, be halved.

During the last few years much interest has been felt in the reduction to an absolute standard of measurements of electro-motive force, current, resistance, etc., and to this end many laborious investigations have been undertaken. The subject is one that has engaged a good deal of my own attention, and I should naturally have felt inclined to dilate upon it, but that I feel it to be too abstruse and special to be dealt with in detail upon an occasion like the present. As regards resistance, I will merely remind you that the recent determinations have shown a so greatly improved agreement, that the Conference of Electricians assembled at Paris, in May, have felt themselves justified in defining the ohm for practical use as the resistance of a column of mercury of 0° C., one square millimetre in section, and 106 centimetres in length—a definition differing by a little more than one per cent. from that arrived at twenty years ago by a committee of this Association.

A standard of resistance once determined upon can be embodied in a 'resistance coil,' and copied without much trouble, and with great accuracy. But in order to complete the electrical system, a second standard of some kind is necessary, and this is not so easily embodied in a permanent form. It might conveniently consist of a standard galvanic cell, capable of being prepared in a definite manner, whose electro-motive force is once for all determined. Unfortunately, most of the batteries in ordinary use are for one reason or another unsuitable for this purpose, but the cell introduced by Mr. Latimer Clark, in which the metals are zinc in contact with saturated zinc sulphate and pure mercury in contact with mercurous sulphate, appears to give satisfactory results. According to my measurements, the electro-motive force of this cell is 1·435 theoretical volts.

We may also conveniently express the second absolute electrical measurement necessary to the completion of the system by taking advantage of Faraday's law, that the quantity of metal decomposed in an electrolytic cell is proportional to the whole quantity of electricity that passes. The best metal for the purpose is silver, deposited from a solution of the nitrate or of the chlorate. The results recently obtained by Professor Kohlrausch and by myself are in very good agreement, and the conclusion that one ampere flowing for one hour decomposes 4·025 grains of silver, can hardly be in error by more than a thousandth part. This number being known, the silver voltameter gives a ready and very accurate method of measuring currents of intensity, varying from $\frac{1}{10}$ ampere to four or five amperes.

The beautiful and mysterious phenomena attending the discharge of electricity in nearly vacuous spaces have been investigated and in some degree explained by De la Rue, Crookes, Schuster, Moulton, and the lamented Spottiswoode, as well as by various able foreign experimenters. In a recent research Crookes has sought the origin of a bright citron-coloured band in the phosphorescent spectrum of certain earths, and after encountering difficulties and anomalies of a most bewildering kind, has succeeded in proving that it is due to yttrium, an element much more widely distributed than had been supposed. A conclusion like this is stated in a few words, but those only who have undergone similar experience are likely to appreciate the skill and perseverance of which it is the final reward.

A remarkable observation by Hall of Baltimore, from which it appeared that the flow of electricity in a conducting sheet was disturbed by magnetic force, has been the subject of much discussion. Mr. Shelford Bidwell has brought forward experiments tending to prove that the effect is of a secondary character, due in the first instance to the mechanical force operating upon the conductor of an electric current when situated in a powerful magnetic field. Mr. Bidwell's view agrees in the main with Mr. Hall's division of the metals into two groups according to the direction of the effect.

Without doubt the most important achievement of the older generation of scientific men has been the establishment and application of the great laws of Thermo-dynamics, or, as it is often called, the Mechanical Theory of Heat. The first law, which asserts that heat and mechanical work can be transformed one into the other at a certain fixed rate, is now well understood by every student of physics, and the number expressing the mechanical equivalent of heat resulting from the experiments of Joule, has been confirmed by the researches of others, and especially of Rowland. But the second law, which practically is even more important than the first, is only now beginning to receive the full appreciation due to it. One reason of this may be found in a not unnatural confusion of ideas. Words do not always lend themselves readily to the demands that are made upon them by a growing science, and I think that the almost unavoidable use of the word equivalent in the statement of the first law is partly responsible for the little attention that is given to the second. For the second law so far contradicts the usual statement of the first, as to assert that equivalents of heat and work are not of equal value. While work can always be converted into heat, heat can only be converted into work under certain limitations. For every practical purpose the work is worth the most, and when we speak of equivalents, we use the word in the same sort of special sense as that in which chemists speak of equivalents of gold and iron. The second law teaches us that the real value of heat, as a source of mechanical power, depends upon the temperature of the body in which it resides; the hotter the body in relation to its surroundings, the more available the heat.

In order to see the relations which obtain between the first and the second law of Thermo-dynamics, it is only necessary for us to glance at the theory of the steam-engine. Not many years ago calculations were plentiful, demonstrating the inefficiency of the steam-engine on the basis of a comparison of the work actually got out of the engine with the mechanical equivalent of the heat supplied to the boiler. Such calculations took into account only the first law of Thermo-dynamics, which deals with the equivalents of heat and work, and have very little bearing upon the practical question of efficiency, which requires us to have regard also to the second law. According to that law the fraction of the total energy which can be converted into work depends upon the relative temperatures of the boiler and condenser; and it is, therefore, manifest that, as the temperature of the boiler cannot be raised indefinitely, it is impossible to utilise all the energy which, according to the first law of Thermo-dynamics, is resident in the coal.

On a sounder view of the matter, the efficiency of the steam-engine is found to be so high, that there is no great margin remaining for improvement. The higher initial temperature possible in the gas engine opens out much wider possibilities, and many good judges look forward to a time when the steam-engine will have to give way to its younger rival.

To return to the theoretical question, we may say with Sir W. Thomson, that though energy cannot be destroyed, it ever tends to be dissipated, or to pass from more available to less available forms. No one who has grasped this principle can fail to recognise its immense importance in the system of the Universe. Every change—chemical, thermal, or mechanical—which takes place, or can take place, in Nature does so, at the cost of a certain amount of available energy. If, therefore, we wish to inquire whether or not a proposed transformation can take place, the question to be considered is whether its occurrence would involve dissipation of energy. If not, the transformation is (under the circumstances of the case) absolutely excluded. Some years ago, in a lecture at the Royal Institution, I endeavoured to draw the attention of chemists to the importance of the principle of dissipation in relation to their science, pointing out the error of the usual assumption that a general criterion is to be found in respect of the development of heat. For example, the solution of a salt in water is, if I may be allowed the phrase, a downhill transformation. It involves dissipation of energy, and can therefore go forward; but in many cases it is associated with the absorption rather than with the development of heat. I am glad to take advantage of the present opportunity in order to repeat my recommendation, with an emphasis justified by actual achievement. The foundations laid by Thomson now bear an edifice of no mean proportions, thanks to the labours of several physicists, among whom must be especially mentioned Willard Gibbs and Helmholtz. The former has elaborated a theory of the equilibrium of heterogeneous substances, wide in its principles, and we cannot doubt far-reaching in its consequences. In a series of masterly papers Helmholtz has developed the conception of *free energy* with very important applications to the theory of the galvanic cell. He points out that the mere tendency to solution bears in some cases no small proportion to the affinities more usually reckoned chemical, and contributes largely to the total electro-motive force. Also in our own country Dr. Alder Wright has published some valuable experiments relating to the subject.

From the further study of electrolysis we may expect to gain improved views as to the nature of the chemical reactions, and of the forces concerned in bringing them about. I am not qualified—I wish I were—to speak to you on recent progress in general chemistry. Perhaps my feelings towards a first love may blind me, but I cannot help thinking that the next great advance, of which we have already some foreshadowing, will come on this side. And if I might without presumption venture a word of recommendation, it would be in favour of a more minute study of the simpler chemical phenomena.

Under the head of scientific mechanics it is principally in relation to fluid motion that advances may be looked for. In speaking upon this subject I must limit myself almost entirely to experimental work. Theoretical hydro-dynamics, however important and interesting to the mathematician, are eminently unsuited to oral exposition. All I can do to attenuate an injustice, to which theorists are pretty well accustomed, is to refer you to the admirable reports of Mr. Hicks, published under the auspices of this Association.

The important and highly practical work of the late Mr. Froude in relation to the propulsion of ships is doubtless known to most of you. Recognising the fallacy of views then widely held as to the nature of the resistance to be overcome, he showed to demonstration that, in the case of fair-shaped bodies, we have to deal almost entirely with resistance dependent upon skin friction, and at high speeds upon the generation of surface waves by which energy is carried off. At speeds which are moderate in relation to the size of the ship, the resistance is practically dependent upon skin friction only. Although Professor Stokes and other mathematicians had previously published calculations pointing to the same conclusion, there can be no doubt that the view generally entertained was very different. At the first meeting of the Association which I ever attended, as an intelligent listener, at Bath in 1864, I well remember the surprise which greeted a statement by Rankine that he regarded skin friction as the only legitimate resistance to the progress of a well-designed ship. Mr. Froude's experiments have set the question at rest in a manner satisfactory to those who had little confidence in theoretical prevision.

In speaking of an explanation as satisfactory in which skin friction is accepted as the cause of resistance, I must guard myself against being supposed to mean that the nature of skin friction is itself well understood. Although its magnitude varies with the smoothness of the surface, we have no reason to think that it would disappear at any degree of smoothness consistent with an ultimate molecular structure. That it is connected with fluid viscosity is evident enough, but the *modus operandi* is still obscure.

Some important work bearing upon the subject has recently been published by Professor O. Reynolds, who has investigated the flow of water in tubes as dependent upon the velocity of motion and upon the size of the bore. The laws of motion in capillary tubes, discovered experimentally by Poiseuille, are in complete harmony with theory. The resistance varies as the velocity, and depends in a direct manner upon the constant of viscosity. But when we come to the larger pipes and higher velocities with which engineers usually have to deal, the theory which presupposes a regularly stratified motion evidently ceases to be applicable, and the problem becomes essentially identical with that of skin friction in relation to ship propulsion. Professor Reynolds has traced with much success the passage from the one state of things to the other, and has proved the applicability under these complicated conditions of the general laws of dynamical similarity as adapted to viscous fluids by Professor Stokes. In spite of the difficulties which beset both the theoretical and experimental treatment, we may hope to attain before long to a better understanding of a subject which is certainly second to none in scientific as well as practical interest.

As also closely connected with the mechanics of viscous fluids, I must not forget to mention an important series of experiments upon the friction of oiled surfaces, recently executed by Mr. Tower for the Institution of Mechanical Engineers. The results go far towards upsetting some ideas hitherto widely admitted. When the lubrication is adequate, the friction is found to be nearly independent of the load, and much smaller than is usually supposed, giving a coefficient as low as $\frac{1}{1000}$. When the layer of oil is well formed, the pressure between the solid surfaces is really borne by the fluid, and the work lost is spent in shearing, that is, in causing one stratum of the oil to glide over another.

In order to maintain its position, the fluid must possess a certain degree of viscosity, proportionate to the pressure; and even when this condition is satisfied, it would appear to be necessary that the layer should be thicker on the ingoing than on the outgoing side. We may, I believe, expect from Professor Stokes a further elucidation of the processes involved. In the meantime, it is obvious that the results already obtained are of the utmost value, and fully justify the action of the Institution in devoting a part of its resources to experimental work. We may hope indeed that the example thus wisely set may be followed by other public bodies associated with various departments of industry.

I can do little more than refer to the interesting observations of Prof. Darwin, Mr. Hunt, and M. Forel on Ripplemark. The processes concerned would seem to be of a rather intricate character, and largely dependent upon fluid viscosity. It may be noted indeed that most of the still obscure phenomena of hydro-dynamics require for their elucidation a better comprehension of the laws of viscous motion. The subject is one which offers peculiar difficulties. In some problems in which I have lately been interested, a circulating motion presents itself of the kind which the mathematician excludes from the first when he is treating of fluids destitute altogether of viscosity. The intensity of this motion proves, however, to be independent of the coefficient of viscosity, so that it cannot be correctly dismissed from consideration as a consequence of a supposition that the viscosity is infinitely small. The apparent breach of continuity can be explained, but it shows how much care is needful in dealing with the subject, and how easy it is to fall into error.

The nature of gaseous viscosity, as due to the diffusion of momentum, has been made clear by the theoretical and experimental researches of Maxwell. A flat disc moving in its own plane between two parallel solid surfaces is impeded by the necessity of shearing the intervening layers of gas, and the magnitude of the hindrance is proportional to the velocity of the motion and to the viscosity of the gas, so that under similar circumstances this effect may be taken as a measure, or rather definition, of the viscosity. From the dynamical theory of gases, to the development of which he contributed so much, Maxwell drew the startling conclusion that the viscosity of a gas should be independent of its density,—that within wide limits the resistance to the moving disc should be scarcely diminished by pumping out the gas, so as to form a partial vacuum. Experiment fully confirmed this theoretical anticipation, —one of the most remarkable to be found in the whole history of science, and proved that the swinging disc was retarded by the gas, as much when the barometer stood at half an inch as when it stood at thirty inches. It was obvious, of course, that the law must have a limit, that at a certain point of exhaustion the gas must begin to lose its power; and

I remember discussing with Maxwell, soon after the publication of his experiments, the whereabouts of the point at which the gas would cease to produce its ordinary effect. His apparatus, however, was quite unsuited for high degrees of exhaustion, and the failure of the law was first observed by Kundt and Warburg, as pressures below 1 mm. of mercury. Subsequently the matter has been thoroughly examined by Crookes, who extended his observations to the highest degrees of exhaustion as measured by MacLeod's gauge. Perhaps the most remarkable results relate to hydrogen. From the atmospheric pressure of 760 mm. down to about ½ mm. of mercury the viscosity is sensibly constant. From this point to the highest vacua, in which less than one-millionth of the original gas remains, the coefficient of viscosity drops down gradually to a small fraction of its original value. In these vacua Mr. Crookes regards the gas as having assumed a different, ultra-gaseous, condition; but we must remember that the phenomena have relation to the other circumstances of the case, especially the dimensions of the vessel, as well as to the condition of the gas.

Such an achievement as the prediction of Maxwell's law of viscosity has, of course, drawn increased attention to the dynamical theory of gases. The success which has attended the theory in the hands of Clausius, Maxwell, Boltzmann and other mathematicians, not only in relation to viscosity, but over a large part of the entire field of our knowledge of gases, proves that some of its fundamental postulates are in harmony with the reality of Nature. At the same time, it presents serious difficulties; and we cannot but feel that while the electrical and optical properties of gases remain out of relation to the theory, no final judgment is possible. The growth of experimental knowledge may be trusted to clear up many doubtful points, and a younger generation of theorists will bring to bear improved mathematical weapons. In the meantime we may fairly congratulate ourselves on the possession of a guide which has already conducted us to a position which could hardly otherwise have been attained.

In Optics attention has naturally centred upon the spectrum. The mystery attaching to the invisible rays lying beyond the red has been fathomed to an extent that, a few years ago, would have seemed almost impossible. By the use of special photographic methods Abney has mapped out the peculiarities of this region with such success that our knowledge of it begins to be comparable with that of the parts visible to the eye. Equally important work has been done by Langley, using a refined invention of his own based upon the principle of Siemens' pyrometer. This instrument measures the actual energy of the radiation, and thus expresses the effects of various parts of the spectrum upon a common scale, independent of the properties of the eye and of sensitive photographic preparations. Interesting results have also been obtained by Becquerel, whose method is founded upon a curious action of the ultra-red rays in enfeebling the light emitted by phosphorescent substances. One of the most startling of Langley's conclusions relates to the influence of the atmosphere in modifying the quality of solar light. By the comparison of observations made through varying thicknesses of air, he shows that the atmospheric absorption tells most upon the light of high refrangibility; so that, to an eye situated outside the atmosphere, the sun would present a decidedly bluish tint. It would be interesting to compare the experimental numbers with the law of scattering of light by small particles given some years ago as the result of theory. The demonstration by Langley of the inadequacy of Cauchy's law of dispersion to represent the relation between refrangibility and wave-length in the lower part of the spectrum must have an important bearing upon optical theory.

The investigation of the relation of the visible and ultra-violet spectrum to various forms of matter has occupied the attention of a host of able workers, among whom none have been more successful than my colleagues at Cambridge, Professors Liveing and Dewar. The subject is too large both for the occasion and for the individual, and I must pass it by. But, as more closely related to Optics proper, I cannot resist recalling to your notice a beautiful application of the idea of Doppler to the discrimination of the origin of certain lines observed in the solar spectrum. If a vibrating body have a general motion of approach or recession, the waves emitted from it reach the observer with a frequency which in the first case exceeds, and in the second case falls short of, the real frequency of the vibrations themselves. The consequence is that, if a glowing gas be in motion in the line of sight, the spectral lines are thereby displaced from the position that they would occupy were the gas at rest—a principle which, in the hands of Huggins and others, has led to a determination of the motion of certain fixed stars relatively to the solar system. But the sun is itself in rotation, and thus the position of a solar spectral line is slightly different according as the light comes from the advancing or from the retreating limb. This displacement was, I believe, first observed by Thollon; but what I desire now to draw attention to is the application of it by Cornu to determine whether a line is of solar or atmospheric origin. For this purpose a small image of the sun is thrown upon the slit of the spectroscope, and caused to vibrate two or three times a second, in such a manner that the light entering the instrument comes alternately from the advancing and retreating limbs. Under these circumstances a line due to absorption within the sun appears to tremble, as the result of slight alternately opposite displacements. But if the seat of the absorption be in the atmosphere, it is a matter of indifference from what part of the sun the light originally proceeds, and the line maintains its position in spite of the oscillation of the image upon the slit of the spectroscope. In this way Cornu was able to make a discrimination which can only otherwise be effected by a difficult comparison of appearances under various solar altitudes.

The instrumental weapon of investigation, the spectroscope itself, has made important advances. On the theoretical side, we have for our guidance the law that the optical power in gratings is proportional to the total number of lines accurately ruled, without regard to the degree of closeness, and in prisms that it is proportional to the thickness of glass traversed. The magnificent gratings of Rowland are a new power in the hands of the spectroscopist, and as triumphs of mechanical art seem to be little short of perfection. In our own report for 1882, Mr. Mallock has described a machine, constructed by him, for ruling large diffraction gratings, similar in some respects to that of Rowland.

The great optical constant, the velocity of light, has been the subject of three distinct investigations by Cornu, Michelson, and Forbes. As may be supposed, the matter is of no ordinary difficulty, and it is therefore not surprising that the agreement should be less decided than could be wished. From their observations, which were made by a modification of Fizeau's method of the toothed wheel, Young and Forbes drew the conclusion that the velocity of light *in vacuo* varies from colour to colour, to such an extent that the velocity of blue light is nearly two per cent. greater than that of red light. Such a variation is quite opposed to existing theoretical notions, and could only be accepted on the strongest evidence. Mr. Michelson, whose method (that of Foucault) is well suited to bring into prominence a variation of velocity with wave length, informs me that he has recently repeated his experiments with special reference to the point in question, and has arrived at the conclusion that no variation exists comparable with that asserted by Young and Forbes. The actual velocity differs little from that found from his first series of experiments, and may be taken to be 299,800 kilometres per second.

It is remarkable how many of the playthings of our childhood give rise to questions of the deepest scientific interest. The top is, or may be understood, but a complete comprehension of the kite and of the soap-bubble would carry us far beyond our present stage of knowledge. In spite of the admirable investigations of Plateau, it still remains a mystery why soapy water stands almost alone among fluids as a material for bubbles. The beautiful development of colour was long ago ascribed to the interference of light, called into play by the gradual thinning of the film. In accordance with this view the tint is determined solely by the thickness of the film, and the refractive index of the fluid. Some of the phenomena are however so curious, as to have led excellent observers like Brewster to reject the theory of thin plates, and to assume the secretion of various kinds of colouring matter. If the rim of a wine-glass be dipped in soapy water, and then held in a vertical position, horizontal bands soon begin to show at the top of the film, and extend themselves gradually, downwards. According to Brewster these bands are not formed by the 'subsidence and gradual thinning of the film,' because they maintain their horizontal position when the glass is turned round its axis. The experiment is both easy and interesting; but the conclusion drawn from it cannot be accepted. The fact is that the various parts of the film cannot quickly alter their thickness, and hence when the glass is rotated they re-arrange themselves in order of superficial density, the thinner parts floating up over, or though, the thicker parts. Only thus can the tendency be satisfied for the centre of gravity to assume the lowest possible position.

When the thickness of a film falls below a small fraction of the length of a wave of light, the colour disappears and is replaced by an intense blackness. Professors Reinold and Rücker have recently made the remarkable observation that the whole of the black region, soon after its formation, is of uniform thickness, the passage from the black to the coloured portions being exceedingly abrupt. By two independent methods they have determined the thickness of the black film to lie between seven and fourteen millionths of a millimetre; so that the thinnest films correspond to about one-seventieth of a wave-length of light. The importance of these results in regard to molecular theory is too obvious to be insisted upon.

The beautiful inventions of the telephone and the phonograph, although in the main dependent upon principles long since established, have imparted a new interest to the study of Acoustics. The former, apart from its uses in every-day life, has become in the hands of its inventor, Graham Bell, and of Hughes, an instrument of first-class scientific importance. The theory of its action is still in some respects obscure, as is shown by the comparative failure of the many attempts to improve it. In connection with some explanations that have been offered, we do well to remember that molecular changes in solid masses are inaudible in themselves, and can only be manifested to our ears by the generation of a to and fro motion of the external surface extending over a sensible area. If the surface of a solid remains undisturbed, our ears can tell us nothing of what goes on in the interior.

In theoretical acoustics progress has been steadily maintained, and many phenomena, which were obscure twenty or thirty years ago, have since received adequate explanation. If some important practical questions remain unsolved, one reason is that they have not yet been definitely stated. Almost everything in connection with the ordinary use of our senses presents peculiar difficulties to scientific investigation. Some kinds of information with regard to their surroundings are of such paramount importance to successive generations of living beings, that they have learned to interpret indications which, from a physical point of view, are of the slenderest character. Every day we are in the habit of recognising, without much difficulty, the quarter from which a sound proceeds, but by what steps we attain that end has not yet been satisfactorily explained. It has been proved that when proper precautions are taken we are unable to distinguish whether a pure tone (as from a vibrating tuning fork held over a suitable resonator) comes to us from in front or from behind. This is what might have been expected from an *à priori* point of view; but what would not have been expected is that with almost any other sort of sound, from a clap of the hands to the clearest vowel sound, the discrimination is not only possible but easy and instinctive. In these cases it does not appear how the possession of two ears helps us, though there is some evidence that it does; and even when sounds come to us from the right or left, the explanation of the ready discrimination which is then possible with pure tones, is not so easy as might at first appear. We should be inclined to think that the sound was heard much more loudly with the ear that is turned towards than with the ear that is turned from it, and that in this way the direction was recognised. But if we try the experiment, we find that, at any rate with notes near the middle of the musical scale, the difference of loudness is by no means so very great. The wave-lengths of such notes are long enough in relation to the dimensions of the head to forbid the formation of anything like a sound shadow in which the averted ear might be sheltered.

In concluding this imperfect survey of recent progress in physics, I must warn you emphatically that much of great importance has been passed over altogether. I should have liked to speak to you of those far reaching speculations, especially associated with the name of Maxwell, in which light is regarded as a disturbance in an electro-magnetic medium. Indeed, at one time, I had thought of taking the scientific work of Maxwell as the principal theme of this address. But, like most men of genius, Maxwell delighted in questions too obscure and difficult for hasty treatment, and thus much of his work could hardly be considered upon such an occasion as the present. His biography has recently been published, and should be read by all who are interested in science and in scientific men. His many-sided character, the quaintness of his humour, the penetration of his intellect, his simple but deep religious feeling, the affection between son and father, the devotion of husband to wife, all combine to form a rare and fascinating picture. To estimate rightly his influence upon the present state of science, we must regard not only the work that he executed himself, important as that was, but also the ideas and the spirit which he communicated to others. Speaking for myself as one who in a special sense entered into his labours, I should find it difficult to express adequately my feeling of obligation. The impress of his thoughts may be recognised in much of the best work of the present time. As a teacher and examiner he was well acquainted with the almost universal tendency of uninstructed minds to elevate phrases above things: to refer, for example, to the principle of the conservation of energy for an explanation of the persistent rotation of a fly-wheel, almost in the style of the doctor in 'Le Malade Imaginaire,' who explains the fact that opium sends you to sleep by its soporific virtue. Maxwell's endeavour was always to keep the facts in the foreground, and to his influence, in conjunction with that of Thomson and Helmholtz, is largely due that elimination of unnecessary hypothesis which is one of the distinguishing characteristics of the science of the present day.

In speaking unfavourably of superfluous hypothesis, let me not be misunderstood. Science is nothing without generalisations. Detached and ill-assorted facts are only raw material, and in the absence of a theoretical solvent, have but little nutritive value. At the present time and in some departments, the accumulation of material is so rapid that there is danger of indigestion. By a fiction as remarkable as any to be found in law, what has once been published, even though it be in the Russian language, is usually spoken of as 'known,' and it is often forgotten that the rediscovery in the library may be a more difficult and uncertain process than the first discovery in the laboratory. In this matter we are greatly dependent upon annual reports and abstracts, issued principally in Germany, without which the search for the discoveries of a little-known author would be well-nigh hopeless. Much useful work has been done in this direction in connection with our Association. Such critical reports as those upon Hydro-dynamics, upon Tides, and upon Spectroscopy, guide the investigator to the points most requiring attention, and in discussing past achievements contribute in no small degree to future progress. But though good work has been done, much yet remains to do.

If, as is sometimes supposed, science consisted in nothing but the laborious accumulation of facts, it would soon come to a stand-still, crushed, as it were, under its own weight. The suggestion of a new idea, or the detection of a law, supersedes much that had previously been a burden upon the memory, and by introducing order and coherence facilitates the retention of the remainder in an available form. Those who are acquainted with the writings of the older electricians will understand my meaning when I instance the discovery of Ohm's law as a step by which the science was rendered easier to understand and to remember. Two processes are thus at work side by side, the reception of new material and the digestion and assimilation of the old; and as both are essential, we may spare ourselves the discussion of their relative importance. One remark, however, should be made. The work which deserves, but I am afraid does not always receive, the most credit is that in which discovery and explanation go hand in hand, in which not only are new facts presented, but their relation to old ones is pointed out.

In making oneself acquainted with what has been done in any subject, it is good policy to consult first the writers of highest general reputation. Although in scientific matters we should aim at independent judgment, and not rely too much upon authority, it remains true that a good deal must often be taken upon trust. Occasionally an observation is so simple and easily repeated, that it scarcely matters from whom it proceeds; but as a rule it can hardly carry full weight when put forward by a novice whose care and judgment there has been no opportunity of testing, and whose irresponsibility may tempt him to 'take shots,' as it is called. Those who have had experience in accurate work know how easy it would be to save time and trouble by omitting precautions and passing over discrepancies, and yet, even without dishonest intention, to convey the impression of conscientious attention to details. Although the most careful and experienced cannot hope to escape occasional mistakes, the effective value of this kind of work depends much upon the reputation of the individual responsible for it.

In estimating the present position and prospects of experimental science, there is good ground for encouragement. The multiplication of laboratories gives to the younger generation opportunities such as have never existed before, and which excite the envy of those who have had to learn in middle life much that now forms part of an undergraduate course. As to the management of such institutions there is room for a healthy difference of opinion. For many kinds of original work, especially in connection with accurate measurement, there is need of expensive apparatus; and it is often difficult to persuade a student to do his best with imperfect appliances when he knows that by other means a better result could be attained with greater facility. Nevertheless it seems to me important to discourage too great reliance upon the instrument maker. Much of the best original work has been done with the homeliest appliances; and the endeavour to turn to the best account the means that may be at hand develops ingenuity and resource more than the most elaborate determinations with ready-made instruments. There is danger otherwise that the experimental education of a plodding student should be too mechanical and artificial, so that he is puzzled by small changes of apparatus much as many school-boys are puzzled by a transposition of the letters in a diagram of Euclid.

From the general spread of a more scientific education, we are warranted in expecting important results. Just as there are some brilliant literary men with an inability, or at least a distaste practically amounting to inability, for scientific ideas, so there are a few with scientific tastes whose imaginations are never touched by merely literary studies. To save these from intellectual stagnation during several important years of their lives is something gained; but the thorough-going advocates of scientific education aim at much more. To them it appears strange, and almost monstrous, that the dead languages should hold the place they do in general education; and it can hardly be denied that their supremacy is the result of routine rather than of argument. I do not, myself, take up the extreme position. I doubt whether an exclusively scientific training would be satisfactory; and where there is plenty of time and a literary aptitude I can believe that Latin and Greek may make a good foundation. But it is useless to discuss the question upon the supposition that the majority of boys attain either to a knowledge of the languages or to an appreciation of the writings of the ancient authors. The contrary is notoriously the truth; and the defenders of the existing system usually take their stand upon the excellence of its discipline. From this point of view there is something to be said. The laziest boy must exert himself a little in puzzling out a sentence with grammar and dictionary, while instruction and supervision are easy to organise and not too costly. But when the case is stated plainly, few will agree that we can afford so entirely to disregard results. In after life the intellectual energies are usually engrossed with business, and no further opportunity is found for attacking the difficulties which block the gateways of knowledge. Mathematics, especially, if not learned young, are likely to remain unlearned. I will not further insist upon the educational importance of mathematics and science, because with respect to them I shall probably be supposed to be prejudiced. But of modern languages I am ignorant enough to give value to my advocacy. I believe that French and German, if properly taught, which I admit they rarely are at present, would go far to replace Latin and Greek from a disciplinary point of view, while the actual value of the acquisition would, in the majority of cases, be incomparably greater. In half the time usually devoted, without success, to the classical languages, most boys could acquire a really serviceable knowledge of French and German. History and the serious study of English literature, now shamefully neglected, would also find a place in such a scheme.

There is one objection often felt to a modernised education, as to which a word may not be without use. Many excellent people are afraid of science as tending towards materialism. That such apprehension should exist is not surprising, for unfortunately there are writers, speaking in the name of science, who have set themselves to foster it. It is true that among scientific men, as in other classes, crude views are to be met with as to the deeper things of Nature; but that the life-long beliefs of Newton, of Faraday, and of Maxwell, are inconsistent with the scientific habit of mind, is surely a proposition which I need not pause to refute. It would be easy, however, to lay too much stress upon the opinions of even such distinguished workers as these. Men, who devote their lives to investigation, cultivate a love of truth for its own sake, and endeavour instinctively to clear up, and not, as is too often the object in business and politics, to obscure a difficult question. So far the opinion of a scientific worker may have a special value; but I do not think that he has a claim, superior to that of other educated men, to assume the attitude of a prophet. In his heart he knows that underneath the theories that he constructs there lie contradictions which he cannot reconcile. The higher mysteries of being, if penetrable at all by human intellect, require other weapons than those of calculation and experiment.

Without encroaching upon grounds appertaining to the theologian and the philosopher, the domain of natural science is surely broad enough to satisfy the wildest ambition of its devotees. In other departments of human life and interest, true progress is rather an article of faith than a rational belief; but in science a retrograde movement is, from the nature of the case, almost impossible. Increasing knowledge brings with it increasing power, and great as are the triumphs of the present century, we may well believe that they are but a foretaste of what discovery and invention have yet in store for mankind. Encouraged by the thought that our labours cannot be thrown away, let us redouble our efforts in the noble struggle. In the Old World and in the New, recruits must be enlisted to fill the place of those whose work is done. Happy should I be if, through this visit of the Association, or by any words of mine, a larger measure of the youthful activity of the West could be drawn into this service. The work may be hard, and the discipline severe; but the interest never fails, and great is the privilege of achievement.

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